capacity, just so surely can we obtain the best results in education only by such elasticity in our methods as makes possible the adjustment of the educational procedure to each student's capacity, peculiarities and needs.

And finally, I would speak for a much wider adoption and extension of the method of research in medical education. The daily practise of medicine, for which we are preparing most of our pupils, is research of the highest order and the most difficult type. We train the neuro-muscular apparatus and the special senses to efficiency in any particular direction, by their constant exercise in that activity or direction. How can we better train the mental facilities for research at the bedside than by their exercise in research, in laboratory and clinic? No one would deny that a certain body of fundamental facts and principles must be memorized by every medical student, and facility in certain technical procedures ought to be acquired, but if we hope to arouse in the student a real enthusiasm for his work, and to develop his power of independent initiative and accomplishment in the setting and the solving of problems, it can be done only—certainly most effectively—by setting him to the task of solving problems within his capacity, involving factors within his control, his work being carried on under intelligent, wise supervision. The problems of pathology are peculiarly well adapted for this purpose. They may be so selected as to have to do with materials and factors within the scope of his ability and training, and they are of interest to him because he can readily see their bearing on clinical medicine, for which he feels he is fitting himself. If he is to have zeal in their pursuit, however, it must not be the threshing over of old straw, but new problems whose solution he feels may constitute a real contribution, however small, to medical science. If he can then present the results of his efforts to a dignified, earnest group of his elders, such as assemble at the meetings of this society, it means for him a generation of enthusiasm, a development of real power, such as no other educational method can produce.

This society has great reason to be proud of the subsequent work of many men and women who found here their first opportunity and their best inspiration. It is certain to have increasing cause for gratification, as the years go on, in the results of this phase of its activities.

JOHN MILTON DODSON

PLANT MORPHOLOGY[1]

I PROPOSE to deal with some aspects of the study of plant morphology. In doing so I shall not accept any definition of morphology that would separate it artificially from other departments of botany. I regard the aim of plant morphology as the study and scientific explanation of the form, structure, and development of plants. This abandons any sharp separation of morphology and physiology, and claims for morphology a wider scope than has been customary for the past fifty years. During this period the problem of morphology has been recognized as being "a purely historical one," "perfectly distinct from any of the questions with which physiology has to do," its aim being "to reconstruct the evolutionary tree." The limitation of the purpose of morphological study, expressed in these phrases from the admirable addresses delivered to this section by Dr. Scott and Professor Bower some twenty years ago, was due to the in-

[1] From the address of the president of the Section of Botany, Manchester meeting of the British Association for the Advancement of Science.

fluence of the theory of descent. I fully recognize the interest of the phyletic ideal, but am unable to regard it as the exclusive, or perhaps as the most important, object of morphological investigation. To accept the limitation of morphology to genealogical problems is inconsistent with the progress of this branch of study before the acceptance of the theory of descent, and leaves out many of the most important problems that were raised and studied by the earlier morphologists.

In the history of morphology, after it had ceased to be the handmaid of the systematic botany of the higher plants, we may broadly distinguish an idealistic period, a developmental period, and a phyletic period. The period of developmental morphology, the most fruitful and the most purely inductive in our science, was characterized by an intimate connection between morphological and physiological work. Among its contributions were studies of development or "growth histories" of whole plants and their members. These were carried out, in part at least, in order to investigate the nature of development, and such general problems found their expression at the close of the period in the "Allgemeine Morphologie" of Hofmeister. The "Origin of Species" took some years before it affected the methods and aims of botanical work. Then its effect on morphology was revolutionary, and, as in all revolutions, some of the best elements of the previous *régime* were temporarily obscured. This excessive influence of the theory of descent upon morphology did not come from Darwin himself, but from his apostle Haeckel, who gave a very precise expression to the idea of a genealogical grouping of animals and plants, illustrated by elaborate hypothetical phylogenetic trees. Such ideas rapidly dominated morphological work, and we find a special "phylogenetic method" advocated by Strasburger. The persistence of the phyletic period to the present time is shown, not only in the devotion of morphology to questions of relationship, but in the attempts made to base homologies upon descent only. Lankester's idea of homogeny can be traced to the influence of Haeckel, and nothing shows the consistency of phyletic morphology to its clear but somewhat narrow ideal so plainly as the repeated attempts to introduce into practise a sharp distinction between homogeny and homoplasy.

Professor Bower, in his address last year and in other papers, has dealt illuminatingly with the aims and methods of phyletic morphology. I need only direct attention to some aspects of the present position of this, which bear on causal morphology. The goal of phyletic morphology has throughout been to construct the genealogical tree of the vegetable kingdom. In some ways this seems farther off than ever. Phyletic work has been its own critic, and the phylogeny of the genealogical tree, since that first very complete monophyletic one by Haeckel, affords a clear example of a reduction series. The most recent and trustworthy graphic representations of the inter-relationships of plants look more like a bundle of sticks than a tree. Consider for a moment our complete ignorance of the inter-relationships of the Algæ, Bryophyta, and Pteridophyta. Regarding the Algæ we have no direct evidence, but the comparative study of existing forms has suggested parallel developments along four or more main lines from different starting-points in a very simple unicellular ancestry. We have no clue, direct or indirect, to the ancestral forms of the Bryophyta, and it is an open question whether there may not be as many

parallel series in this group as in the Algæ. The Pteridophyta seem a better case, for we have direct evidence from fossil plants as well as the comparison of living forms to assist us. Though palæobotany has added the Sphenophyllales to the existing groups of vascular cryptogams and has greatly enlarged our conceptions of the others, there is no proof of how the great groups are related to one another. As in the Bryophyta, they may represent several completely independent parallel lines. There is no evidence as to what sort of plants the Pteridophyta were derived from, and in particular none that relates them to any group of Bryophyta or Algæ. I do not want to labor the argument, but much the same can be said of the seed-plants, though there is considerable evidence and fairly general agreement as to some Gymnosperms having come from ancient Filicales. The progress of phyletic work has thus brought into relief the limitations of the possible results and the inherent difficulties. As pointed out by Professor Bower, we can hope for detailed and definite results only in particularly favorable cases, like that of the Filicales.

The change of attitude shown in recent phyletic work towards "parallel developments in phyla which are believed to have been of distinct origin" is even more significant. Prof. Bower spoke of the prevalence of this as an "obstacle to success," and so it is if our aim is purely phyletic. In another way the demonstration of parallel developments constitutes a positive result of great value. Thus Professor Bower's own work has led to the recognition of a number of series leading from the lower to the higher Filicales. By independent but parallel evolutionary paths, from diverse starting-points in the more ancient ferns, such similarity has been reached that systematists have placed the plants of distinct origin in the same genus. In these progressions a number of characters run more or less clearly parallel, so that the final result appears to be due "to a phyletic drift that may have affected similarly a plurality of lines of descent." This conclusion, based on detailed investigation, appears to me to be of far-reaching importance. If a "phyletic drift" in the ferns has resulted in the independent and parallel origin of such characters as dictyostely, the mixed sorus, and the very definite type of sporangium with a vertical annulus and transverse dehiscence, the case for parallel developments in other groups is greatly strengthened. The interest shifts to the causes underlying such progressive changes as appear in parallel developments, and the problem becomes one of causal morphology rather than purely historical.

The study of parallel developments would, indeed, seem likely to throw more light on the morphology of plants than the changes traced in a pure phyletic line, for it leads us to seek for common causes, whether internal or external. We cease to be limited in our comparisons by actual relationship, or forbidden to elucidate the organization in one group by that which has arisen independently in another. Similarly the prohibition against comparing the one generation in the life-cycle with the other falls to the ground, quite apart from any question of whether the alternation is homologous or antithetic. The methods of advance and the causal factors concerned become the important things, and if, for example, light is thrown on the organization of the fern-plant by comparison with the gametophyte of the moss, so much the better. This, however, is frankly to abandon phylogeny as "the only real basis of morphological study,"

and with this any attempt to base homology on homogeny. Many of the homologies that exist between series of parallel development are what have been happily termed homologies of organization; these are sometimes so close as to result in practical identity, at other times so distinct as to be evident homoplasies. The critical study of homologies of organization over as wide an area as possible becomes of primary interest and importance.

Since about the beginning of the present century a change of attitude towards morphological problems has become more and more evident in several ways. It seems to be a phyletic drift affecting simultaneously a plurality of lines of thought. The increasing tendency to look upon problems of development and construction from a causal point of view is seen in the prominence given to what may be termed developmental physiology, and also in what Goebel has called organography. These deal with the same problems from different sides and neither formulates them as they appear to the morphologist. Together with genetics, they indicate the need of recognizing what I prefer to call general or causal morphology.

The problems of causal morphology are not new, though most of them are still unsolved and are difficult to formulate, let alone to answer. As we have seen, they were recognized in the time of developmental morphology, though they have since been almost wholly neglected by morphologists. So far as they have been studied during the phyletic period, it has been from the physiological rather than the morphological side. Still, such problems force themselves upon the ordinary morphologist, and it is from his position that I venture to approach them. I willingly recognize, however, that causal morphology may also be regarded as a department of plant-physiology. In development, which is the essential of the problem, the distinction between morphology and physiology really disappears, even if this distinction can be usefully maintained in the study of the fully developed organism. We are brought up against a fact which is readily overlooked in these days of specialization, that botany is the scientific study of plants.

General morphology agrees with physiology in its aim, being a causal explanation of the plant and not historical. Its problems would remain if the phyletic history were before us in full. In the present state of our ignorance, however, we need not be limited to a physico-chemical explanation of the plant. Modern physiology rightly aims at this so far as possible, but, while successful in some departments, has to adopt other methods of explanation and analysis in dealing with irritability. It is even more obvious that no physico-chemical explanation extends far enough to reach the problems of development and morphological construction. The morphologist must therefore take the complicated form and its genesis in development and strive for a morphological analysis of the developing plant. This is to attack the problem from the other side, and to work back from the phenomena of organization toward concepts of the nature of the underlying substance.

It is to these questions of general morphology with a causal aim (for causal morphology, though convenient, is really too ambitious a name for anything we yet possess) that I wish to ask your attention. All we can do at first is to take up a new attitude towards our problems, and to gather here and there hints upon which new lines of attack may be based. This new attitude is, however, as I have pointed

out, a very old one, and in adopting it we re-connect with the period of developmental morphology. Since the limited time at my disposal forbids adequate reference to historical details, and to the work and thought of many botanists in this field, let me in a word disclaim any originality in trying to express in relation to some morphological problems what seems to me the significant trend, in part deliberate and in part unconscious, of morphology at present. The methods available in causal morphology are the detailed study in selected plants of the normal development and its results, comparison over as wide an area as possible, with special attention to the essential correspondences (homologies of organization) arrived at independently, the study of variations, mutations, and abnormalities in the light of their development, and ultimately critical experimental work. This will be evident in the following attempt to look at some old questions from the causal point of view. I shall take them as suggested by the fern, without confining my remarks to this. The fern presents all the main problems in the morphology of the vegetative organs of the higher plants, and what little I have to say regarding the further step to the seed-habit will come as a natural appendix to its consideration.

Twice in its normal life-history the fern exhibits a process of development starting from the single cell and resulting in the one case in the prothallus and in the other in the fern-plant. For the present we may treat these two stages in the life-history as individuals, their development presenting the same general problems as a plant of, say, Fucus or Enteromorpha, where there is no alternation of generations. How is the morphologist to regard this process of individual development?

In the first place, we seem forced to regard the specific distinctness as holding for the germ as well as the resulting mature plant, however the relation between the germ-cell and the characters of the developed organism is to be explained. We start thus with a conception of specific substance, leaving it quite an open question on what the specific nature depends. This enables us to state the problem of development freed from all considerations of the ultimate uses of the developed structure. The course of development to the adult condition can be looked upon as the manifestation of the properties of the specific substance under certain conditions. This decides our attitude as morphologists to the functions of the plant and to teleology. Function does not concern us except in so far as it is found to enter as a causal factor into the process of development. Similarly, until purpose can be shown to be effective as a causal factor it is merely an unfortunate expression for the result attained.

Let me remind you, also, that the individual plant, whether it be unicellular, cœnocytic or multicellular, may behave as a whole at all stages of its development. We see this, for instance, in the germination of *Œdogonium,* in the germination and subsequent strengthening of the basal region in *Fucus* or *Laminaria,* in the moss-plant or fern-plant, or in a dicotyledonous tree. A system of relations is evident in the plant, expressed in the polarity and the mutual influences of the main axis and lateral branches, as well as in the influences exerted on the basal region by the distant growing parts. We thus recognize, in its most general form, the correlation of parts, a concept of proved value in botany.

To some the expression of the observed facts in this way may appear perilously mystical. I do not think so myself. It is true that the nature of the specific substance and of the system of relations is un-

known to us, but it is regarded as a subject for scientific inquiry and further explanation. To recognize fully the complexity of the substance of the plant is not, however, a step towards neo-vitalism, but is perhaps our best safeguard against the dangers of this.

The wholeness of the individual, together with important phenomena of regeneration, has suggested the conclusion that something other than physico-chemical or mechanical laws are concerned in the development of the organism. To this something Driesch applies the name entelechy. Without discussing the vitalistic philosophy of the organism, or other modern phases of philosophic thought that treat life as an entity, it seems worth while to point out that they are based mainly on the consideration of animal development. It would be interesting to inquire into the difficulties that are met with in applying such views to plants, where regeneration in one form or another is the rule rather than the exception, and often does not lead to restitution of the individual. Causal morphology can recognize phenomena of development and of the individual, which are at present beyond physico-chemical explanation, and try to attack them by any methods of investigation that seem practicable, without begging the main question at the outset and then proceeding deductively. To assume any special inner director of development. be it entelechy or vital force, is to cut the knot that may ultimately be untied.

The previous experience of botany in the time of nature-philosophy may well make us cautious of solving our difficulties by the help of any new biological philosophy. On the other hand, cooperation between biology and philosophic thought is highly desirable. In this connection I should like to refer to an idea contained in Prof. Alexander's paper on the basis of realism. He suggests that there is only one matrix from which all qualities arise, and that (without introducing any fresh stuff of existence) the secondary qualities, life, and at a still higher level, mind, emerge by some grouping of the elements within the matrix. The development of this idea as it applies to life would appear to offer a real point of contact between inductive biological work and philosophy.

To return to our plant, its development, with increase in size and progressive complexity of external form and internal structure, must be considered. The power of continued development possessed by most plants and wanting in most animals makes comparison between the two kingdoms difficult. That there is no fundamental difference between the continued and the definitely limited types of embryogeny is, however, shown by plants themselves. The bryophyte sporogonium is a clear example of the latter, while the fern sporophyte is one of many examples of the former. A difference less commonly emphasized is that in the sporogonium (as in the higher animals) the later stages of development proceed by transformation of the whole of the embryo into the mature or adult condition; in the fern-plant the apical development results in successive additions of regions which then attain their mature structure by transformation of the meristematic tissue.

These distinctions are of some importance in considering the generalization originally founded on animal development and known as the biogenetic law. That "the ontogeny is a concise and compressed recapitulation of the phylogeny" is essentially a phyletic conception. It has been more or less criticized and challenged by some distinguished zoologists, and has always been difficult to apply to plants. If we avoid being prejudiced by zoological theory and results, we do not find that the characters of the em-

bryos of plants have given the key to doubtful questions of phylogeny. What help do they give us, for instance, in the algæ or the vascular cryptogams? The extension of the idea of recapitulation to the successively formed regions of the seedling plant requires critical examination; if admitted it is at any rate something different from what the zoologist usually means by this. The facts—as shown, for instance, in a young fern-plant—are most interesting, but can perhaps be better looked at in another way. Development is accompanied by an increase in size of the successively formed leaves and portions of stem, and the process is often cumulative, going on more and more rapidly as the means increase until the adult proportions are attained. The same specific system of relations may thus find different expression in the developing plant as constructive materials accumulate. I do not want to imply that the question is merely a quantitative one; quality of material may be involved, or the explanation may lie still deeper. The point is that the progression is not a necessary one due to some recapitulative memory.

There are some other classes of facts, clearly cognate to normal individual development, that seem to require the causal explanation. I may mention three: (1) Vegetatively produced plants (from bulbils, gemmæ, etc.) tend in their development to pass through stages in elaboration similar to young plants developing from a spore or zygote. The similarities are more striking the smaller the portion of material from which a start is made. (2) Branches may repeat the stages in ontogeny more or less completely also in relation to differences in the nutritive conditions. (3) In the course of continued development there may be a return to the simpler form and structure passed through on the way to the more complex. These cases of parallels to, or reversals of, the normal ontogenetic sequence suggest explanation on causal lines, but are difficulties in the way of phyletic recapitulation; the first two cases can be included under this, while the third seems definitely antagonistic. On the whole, it may be said that recapitulation can not be accepted for plants without further evidence, and that preliminary inquiry disposes us to seek a deeper and more fruitful method of explaining the facts of development.

The development of most plant-individuals starts from a single cell, and when we compare mature forms of various grades of complexity the unicellular condition is also our usual starting-point. What is not so generally recognized or emphasized is the importance of the filament as the primitive construction-form of most plants. I do not use the word primitive in a phyletic sense, nor in the sense of an ideal form, but to indicate a real stage in independent progressions underlying many homologies of organization. I can not develop this fully here, but wide comparison of independent lines of advance suggests that the main types of progress in complexity of the plant-body have involved the elaboration of the single filament with apical growth and with subordinated "branches." It is generally recognized that various groups of algæ show how a solid multicellular axis may come about, not only by the further partition of the segments of the apical cell, but by the congenital cortication of a central filament or the congenital condensation of the subordinated "branches" on to the central axis. The algæ further show the change from the dome-shaped apical cell of a filament to the sunken initial cell with two, three or four sides. The central filament then only appears, if at all, as a subsequent differentiation in the tissue, and the segments serially cut off from the apical cell may or may not bear projecting hair-shoots

or "leaves." The algæ thus attain in independent lines a construction corresponding to that of the plant in liverworts and mosses. In the various parallel series of Bryophyta the filament is not only more or less evident in the ontogeny, but may be regarded as the form underlying both thallus and shoot, between which on this view there is no fundamental distinction. The sporogonium also can be readily regarded as an elaborated filament. While the same interpretation of the fern-prothallus will readily be granted, to think of the fern-plant as the equivalent of an elaborated filament may appear far-fetched. So far from this being the case, I believe that it will be found helpful in understanding the essential morphology of the shoot. In a number of vascular cryptogams and seed-plants, there is actually a filamentous juvenile stage, the suspensor, while the growth by a single apical cell is essentially the same in the fern as in the moss and some algæ.

There follows from this a natural explanation of the growth by a single initial cell so commonly found in plants. The apical cell appears to be the one part of the massive plant-body (for instance, of Laurencia, a moss, or a fern) that persists as a filament; it is a filament one cell long. It may be replaced by a group of initial cells, as we see in some algæ, liverworts and Pteridophyta, and this leads naturally to the small-celled meristems found in most Gymnosperms and Angiosperms. The filamentous condition is then wholly lost, though the system of relations and especially the polarity is maintained throughout all the changes in the apical meristem.

I feel confirmed in regarding the construction of the sporophyte in this fashion by the fact that it fits naturally with the conclusions resulting from the masterly comparative treatment of the embryology of the vascular cryptogams by Professor Bower. These are (1) the primary importance of the longitudinal axis of the shoot, the position of the first root and the foot being variable; (2) the constancy of the position of the stem-apex near the center of the epibasal half of the embryo; (3) the probability that embryos without suspensors have been derived from forms with suspensors, without any example of the converse change. These and other related facts seem to find their morphological explanation in the shoot of the sporophyte being the result of the elaboration of a filament.

The view to which we are thus led is that the uniaxial shoot is a complex whole, equivalent to the axial filament together with its congenitally associated subordinated "branches." This applies to the multicellular plant-bodies found in various independent lines of algæ and Bryophyta, whether they have definite projecting appendages of the nature of leaves or not. The discarding of the distinction between thallus and shoot, which in practise has proved an unsatisfactory one, is no great loss. Even taking the word in the narrower sense of a stem with distinct leaves, the shoots in algæ, liverworts, and mosses, though admittedly independent developments, exhibit an essential correspondence amounting to a homology of organization. The resemblances are not analogies, for it is doubtful whether the "leaves" in the different cases correspond in function. The comparison of the shoot of the sporophyte of a vascular cryptogam with, for example, the shoot of the moss seems equally justifiable. It is only forbidden by strict phyletic morphology, which for our purpose has no jurisdiction. The general agreement as regards the leaf-arrangement between the ferns and the Bryophyta suggests that similar laws will be found to hold in the shoot of both gametophyte and sporophyte. Apart from plagiotropic

shoots, there is a constructionally dorsiventral type of fern-rhizome. The leaves of this alternate as in the leafy liverworts, while the radial type of fern corresponds to the moss-shoot. It is significant that the early leaves of radially constructed ferns usually exhibit a divergence of $\frac{1}{3}$ in the seedling, passing higher up the stem into more complicated arrangements, and the same is the case in mosses. I must not enter into questions of phyllotaxy, but may remark on the hopefulness of attacking it from the study of the simpler shoots of algæ and Bryophyta rather than, as has usually been done, beginning with the flowering plants.

In some ferns (the striking example being *Ceratopteris*) the relation between the segmentation of the apical cell and leaf-production is as definite as in the moss, each segment giving rise to a leaf. This may hold more widely for ferns than is at present demonstrated, and the question deserves thorough reinvestigation to ascertain the facts independently of any theoretical views. That the coincidence of the segmentation of the shoot expressed by the leaf-arrangement and the segmentation of an apical cell is not a necessary one is, however, clearly shown in other ferns, and is obvious in the case of shoots with a small-celled meristem. The two segmentations appear to be determined by some deeper system of relations, which may also be manifested in a cœnocytic plant-body.

In the complication of the uniaxial shoot introduced by branching also there seems to be an advantage in a wide area of comparison. The question most often discussed concerns dichotomous and monopodial branching. If the details of development are to be taken into consideration, the term "dichotomy" has usually been very loosely applied. Apparent dichotomy, the continuation of one shoot by two equally strong ones, is fairly common. But in most cases investigated in detail the branching seems to be really monopodial and the forking due to the equally strong development of a lateral branch close to the main apex, not to the division of the latter. In plants growing by a single initial cell almost the only case of strict dichotomy known is the classic one of Dictyota. The branching of the ferns has been the subject of numerous investigations, but there is a great lack of developmental data. Usually the branches stand in some definite relation to the leaves of the shoot, behind, to one side, or on the leaf-base, itself, the most interesting but least common case being when the branch is in an axillary position. When the mature shoot only is considered, it is possible to argue for the derivation of monopodial branching from dichotomy or the converse. Even the facts obtainable from the mature plant, however, point to the dichotomous branching being a modification of the monopodial, the opposite view appearing to land us in difficulties regarding the morphology of the main shoot. It is unlikely that a dichotomy involving the division of the apical cell occurs in the fern-shoot, and comparison with the Bryophyta confirms the suspicion that the cases of dichotomy are only apparent.

In considering the construction of the shoot we are at present limited to comparison of the normal structure and development. The system of relations in the shoot of the fern, affecting in the first place the distribution of the leaves and secondly that of the branches, appears, however, to be of the same nature as in the independently evolved shoots of Bryophyta and algæ. A morphological analysis based on the simpler examples may lead on to the experimental investigation of the com-

mon construction. The relation that exists between the general construction and the vascular anatomy offers a special and more immediately hopeful problem. Here also, in considering the fern, we are assisted by homologies of organization in other vascular cryptogams and in the more complex Bryophyta, though the algæ are of little help.

In few departments of botany has our knowledge increased so greatly and become so accurate as in that of vascular anatomy. The definiteness of the structures concerned and the fact that they have been almost as readily studied in fossil as in living plants has led to this. Not less important have been the clear concepts first of the bundle system and later of the stele under which the wealth of fact has been brought. Great progress has been made under the influence of phyletic morphology, and anatomy has adopted further conventions of its own and tended to treat the vascular system as if it had an almost independent existence in the plant. The chief method employed has been the comparative study of the mature regions, of necessity in the fossils and by choice in the case of existing plants. I do not. of course, mean to say that we are ignorant of the development of the vascular system, but the variety in it has not been adequately studied in the light of apical development. A gap in our knowledge usually comes between the apical meristem itself and the region with a developed vascular system. It is in this intermediate region that the real differentiation takes place, and the arrangement of the first vascular tracts is then modified by unequal extension of the various parts. The apical differentiation requires separate study for each grade of complexity of the vascular system, even in the same plant.

If we look at the vascular system, not as if it had an independent existence or from the phyletic point of view, but as a differentiation taking place within the body of the individual plant, we can inquire as to the causal factors in the process. A deeper insight into the nature of the stele may be obtained by regarding it as the resultant of a number of factors, as part of the manifestation of the system of relations in development. The first step towards this is the critical consideration of normal developing plants, but so long as the causal influences in the developing substance of a plant remain unchanged the resulting vascular structure will remain constant. Our hope of advance lies in the study of cases where these influences are modified. Herein lies the value of abnormalities, of natural experiments, and the results of experimental interference. Possible influences that have at various times been suggested are functional stimuli, the inductive influence of the older pre-formed parts on the developing region, and formative stimuli of unknown nature proceeding from the developing region. The functional stimuli do not come into play at the time of laying down the vascular tracts, though they may have importance in their maintenance later; the inductive influence of the anatomy of older regions is excluded in the first differentiation of the vascular system in an embryo; we are thus led to attach special importance to the detection of the action of formative stimuli proceeding from the young developing primordia. We have further to take external stimuli into account, though these must act by influencing the internal system of relations.

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I have touched on a number of large questions, any one of which demanded separate treatment. My concern has not, however, been with them individually, but as cognate problems justifying the deliberate

adoption of a causal explanation as the aim of morphological work. I have confined myself to problems bearing on the development and self-construction of the individual, and tried to treat them so as to illustrate the causal attitude and possible lines of attack. Preliminary speculations on the questions considered can at best contain a germ of truth, and must be subsequently adjusted in the light of further facts. I have discussed these questions rather than the smaller modifications in individual development shown in metamorphosis, partly because the latter have of late years been treated from a causal point of view, and partly because I wished to consider questions that immediately affect us as working morphologists.

Did time allow, we should naturally be led to recognize the same change of attitude in biological science toward the problems of the origin of new forms. Questions of bud-variation and mutation are clearly akin to some of those considered, and the whole subject of genetics is a special attempt at a causal explanation of form and structure and the resulting functions. Close cooperation between the morphological analysis of the plant and the genetic analysis attained by the study of hybridization is most desirable. It is especially desirable that both should deal with structure as well as with form, and in the light of individual development.

The causal factors which have determined and guided evolution can be naturally regarded as an extension of the same line of inquiry. The Darwinian theory, and especially the exposition of the principle of natural selection, was the greatest contribution ever made to the causal explanation of the organic world. Strangely enough, it led to a period of morphological work in which the causal aim was almost lost sight of. Why evolution has taken place in certain directions and not in others is a problem to the solution of which causal morphology will contribute. The probability of orthogenesis, both in the animal and vegetable kingdoms, is again coming into prominence, however it is to be explained. When we consider the renewed activity in this field it is well to remember that, just as is the case with causal morphological work, we are picking up a broken thread in the botanical web. Lastly, as if summing up all our difficulties in one, we have the problem of adaptation. In attacking it we must realize that use and purpose have often been assumed rather than proved. We may look to scientific ecological work to help us to estimate the usefulness or the selection value of various characters of the plant. On the other hand, causal morphology may throw light on whether the "adaptation" has not, in some cases at least, arisen before there was a "use" for it. The hopeful sign in the recent study of these greater morphological problems is that the difficulties are being more intensely realized, and that rapid solutions are justly suspect. The more the causal attitude is adopted in ordinary morphological work, the more hope there is of these larger questions being inductively studied rather than argued about.

The causal aim is essentially different from the historical one, but there is no opposition between causal and phyletic morphology. They are rather mutually helpful, for there has been an evolution, not of mature plants, but of specific substances exhibiting development. A deeper insight into the nature of ontogeny is thus bound to be of assistance to phyletic morphology, while the tested results of phyletic work afford most valuable guidance in general causal morphology, though this can not accept any limitation to single lines of descent in its comparisons.

I have tried to bring before you the possibilities of causal morphology partly because the same attention has never been given to it in this country as to other branches of botany, and partly because if morphology be conceived in this broader spirit it need not be said that it has no practical bearing. I should not regard it as a serious disability were the study of purely scientific interest only, but this is not the case. When, if ever, we penetrate into the secrets of organization so far as to be able to modify the organism at will (and genetics has advanced in this direction), the practical possibilities become incalculable.

Probably all of us have reflected on what changes the war may bring to botanical work. It is impossible to forecast this, but I should like to emphasize what my predecessor said in his address last year as to pure science being the root from which applied science must spring. Though results may seem far off, we must not slacken, but redouble our efforts towards the solution of the fundamental problems of the organism. This can be done without any antagonism between pure and applied botany; indeed, there is every advantage in conducting investigations on plants of economic importance. It would be well if every botanist made himself really familiar with some limited portion of applied botany, so as to be able to give useful assistance and advice at need. The stimulus to investigation would amply repay the time required. Even in continuing to devote ourselves to pure botany we can not afford to waste time and energy in purposeless work. It is written in "Alice in Wonderland" that "no wise fish goes anywhere without a porpoise," and this might hang as a text in every research laboratory.

A plant is a very mysterious and wonderful thing, and our business as botanists is to try to understand and explain it as a whole and to avoid being bound by any conventional views of the moment. We have to think of the plant as at once a physico-chemical mechanism and as a living being; to avoid either treating it as something essentially different from non-living matter or forcibly explaining it by the physics and chemistry of to-day. It is an advantage of the study of causal morphology that it requires us to keep the line between these two crudities, a line that may some day lead us to a causal explanation of the developing plant and the beginnings of a single science of botany.

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WHICH OF THE PRESENT MEMBERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE HAVE HELD THE LONGEST CONTINUOUS MEMBERSHIP?

THIS is a question which often comes up at the meetings and several of the old-time members occasionally claim to be the oldest living member.

Mr. F. S. Hazard, the assistant secretary of the association, in going over the list of members, has drawn up the following statement concerning the members now living:

*Hilgard, Eugene Woldemar, Ph.D., LL.D., University of California, Berkeley, Calif. (11.) 1874. B, C, E.

*Würtele, Rev. Louis Campbell, P. O. Drawer E, Acton Vale, Quebec, Canada. (11.) 1875. E, H.

*Paine, Cyrus Fay, 520 East Avenue, Rochester, N. Y. (12.) 1874. A, B.

*Fairbanks, Rev. Henry, Ph.D., St. Johnsbury, Vt. (14.) 1874. A, B, D.

*Wright, Arthur Williams, Ph.D., Yale University, New Haven, Conn. (14.) 1874. A, B.

*Raymond, Rossiter W., Ph.D., LL.D., 29 West 39th St., New York, N. Y. (15.) 1875. B, C, D, E, I, K.

*Abbe, Cleveland, Ph.D., LL.D., U. S. Weather Bureau, Washington, D. C. (16.) 1874. A, B.

*Beal, William James, Ph.D., Sc.D., 40 Sunset Avenue, Amherst, Mass. (17.) 1880. G.